ANADROMOUS FISH RESTORATION STUDY

Ron J Sutton

Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0303: ANADROMOUS FISH RESTORATION STUDY

Final Panel Rating
inadequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

Although one of the technical reviewers rated certain aspects of the proposal as good to excellent, the remainder of the reviews lead to the conclusion that the proposal is inadequate. The proposed work will rely solely on PHABSIM and temperature modeling to test the hypotheses; no field data collection or monitoring is to be performed. The workplan relies on the key assumptions that the flow regime would be implemented, the fall-run Chinook salmon would be able to use the mainstem San Joaquin River as a migration corridor up to Friant Dam, and that flow adjustments could be made, if necessary, depending on the biological response. An underlying assumption in the hypothesis is that temperature, depth and velocity are the limiting factors for establishing fall-run Chinook salmon habitat. Considering the physical conditions in the reach between Friant Dam and the Merced River, and the institutional constraints on managing flows in this reach, these assumptions are very far-reaching, and unlikely to be met in the foreseeable future. In the absence of an implementation and monitoring plan, this proposal is essentially a modeling exercise to determine if a flow regime that mimics the natural regime and that provides an acceptable temperature regime is possible, and there is no direct mechanism in the workplan to test the primary hypothesis. In addition, considerable, detailed hydraulic, hydrologic and

temperature modeling has already been performed by others in this reach over the past decade or more. In fact, it is the primary TSP reviewers understanding that BOR staff in Denver are developing (or have completed development of) a temperature model of this reach. Based on the workplan and citations, the authors appear to have an inadequate knowledge of previous and ongoing work in this reach that could substantially reduce the effort required to carry out the proposed work.

Additional Comments:

Although one of the technical reviewers rated certain aspects of the proposal as good to excellent, the remainder of the reviews lead to the conclusion that the proposal is inadequate. The proposed work will rely solely on PHABSIM and temperature modeling to test the hypotheses; no field data collection or monitoring is to be performed. The workplan relies on the key assumptions that the flow regime would be implemented, the fall−run Chinook salmon would be able to use the mainstem San Joaquin River as a migration corridor up to Friant Dam, and that flow adjustments could be made, if necessary, depending on the biological response. An underlying assumption in the hypothesis is that temperature, depth and velocity are the limiting factors for establishing fall−run Chinook salmon habitat. Considering the physical conditions in the reach between Friant Dam and the Merced River, and the institutional constraints on managing flows in this reach, these assumptions are very far−reaching, and unlikely to be met in the foreseeable future. In the absence of an implementation and monitoring plan, this proposal is essentially a modeling exercise to determine if a flow regime that mimics the natural regime and that provides an acceptable temperature regime is possible, and there is no direct mechanism in the workplan to test the primary hypothesis. In addition, considerable, detailed hydraulic, hydrologic and temperature modeling has already been performed by others in this reach over the past decade or more. In fact, it is the primary TSP reviewers understanding that BOR staff in Denver are developing (or have completed development of) a

temperature model of this reach. Based on the workplan and citations, the authors appear to have an inadequate knowledge of previous and ongoing work in this reach that could substantially reduce the effort required to carry out the proposed work.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

Two of the three external technical reviewers and the panel considered this proposal to have serious technical deficiencies. First, while the proposal does state hypotheses that will be addressed by the modeling, it does not describe how these hypotheses will be tested. Second, the proposed work does not contribute to scientific understanding as expected of CALFED science projects because it is simply a modeling exercise that develops a synthetic flow regime, with no mechanism to test the flow regime; further, the products do not include peer−reviewed publications. The basic premise of the work is also likely to be invalid because it assumes that temperature, velocity, and depth are the limiting factors, and other critical factors such as migration barriers, substrate quality and water supply are assumed not to be present. Third, the proposal does not acknowledge or describe the relationship of the proposed work to the extensive body of recent and on−going work conducted on these topics on this river.

Technical Review #1

proposal title: ANADROMOUS FISH RESTORATION STUDY

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals of this study are clear. The intent is to 1) apply an existing physical habitat model in concert with an existing temperature model to improve temperature conditions and significantly increase the abundance of chinook salmon returning to the San Joaquin River below Friant Dam, and, 2) develop a monitoring and adaptive managment program to be implemented with the flow regime.** **The hypotheses is reasonable yet imprecise. From the proposal it reads:** **“The BoR proposes to develop an instream flow regime that mimics the natural flows which would improve water temperature conditions and significantly increase the numbers of adult returning fall−run chinook in the upper mainstem of the San Joaquin River”** **The hypothesis specifies something the BoR wants to do but it is not stated as a scientific hypothesis (i.e. it is not testable). Aside, the idea is not new as it forms an standard component of most instream flow assessments in California using the tools that have been used for this purpose for nearly two decades.**
Rating	

	fair

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**There is very little information given in this proposal to justify the work and the reveiwer is left filling many information gaps. The proposal does not clear spell out why the consideration of temperature is a concern on the San Joaquin River (particularly with consideration of the multitude of other factors that impact fall run chinook and physical habitat in the system), nor how this proposed work dovetails into the current efforts to manage the instream flow regime. As the work is directed at one river, uses existing methods, the proposed work is not expected to provide a significant new method or approach that can be applied elsewhere.** **A science initiative should produce one of two results to be a valid and useful project. It either has to: 1) contribute directly to an ongoing applied water resource management situation within an established process, or, 2) provide a significant piece of new information or development of promising method or approach to a broad set of circumstances under the auspices of the funding program. It is not clear that this proposal will do either.**
Rating	**poor**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be

useful to decision makers?

Comments	**From the information provided in the proposal there is no evidence that the work is well thought out. The required approach to the modelling work, monitorng program design, and adaptive management process are poorly described yet likely feasible since the models already exist and been applied in many instream flow assessments in California and other locations. Similarly, there is little documentation of the other relevant work that has occurred on the San Joaquin River and how this fits in to current management processes to assess if this will actually be helpful.** **There is insufficient detail in the proposal to demonstrate that enough thought has gone into the work plan and thus the utility for current management is uncertain and questionable.**
Rating	**poor**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The proposal does not provide enough detail to judge the feasibility of the work. Implicitly the operation of existing models appears feasible but the poor description of the monitoring and adaptive management components of the work plan demonstrates a lack of understanding of their expected complexity.** **The modeling componets appear to be in the grasp of the author but the monitorng and adpative managment components do not.**
Rating	**poor**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**The component of work associated with the developing of the monitoring program and adaptive management programs are insufficiently documented and as a result it is not possible to explicitly review.** **From the proposal it appears that there is little to no consideration of the other potential factors complicating the monitoring programs, the expected statistical reliability of inferences, appropriateness of different population metrics, and how inferences need to be derived both from qualitative and quantitative information (i.e. expect low statistical reliability so need a weight of evidence approach). Both the monitoring and adaptive management components are treated as simple and inconsequential. This is a significant oversight that greatly detracts from the expected utility of the work**
Rating	**poor**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The proposal does not provide the reviewer a reasonable explicit understanding of how the utility of products from the proposal will contribute to management or how it will be useful for managers.**
Rating	**poor**

Additional Comments

Comments	**No additional comments becuase of significant concerns already identified.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The proposed investigator does appear to have proven capability to efficiently and effectively utilize the instream flow assessment models and access to expertise required to properly operate the temperature model. This expertise could be applied for definition of what flow and temperature monitoring program would required, if they do not already already exist.** **The proposed investigator does not appear to have proven capability to efficiently and effectively develop the biological monitoring or adaptive management program.**
Rating	**fair**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**Based on the knowledge that the physical habitat simulation and temperature models are pre-existing and the expected detail in the monitoring programs based on the proposal is low and very general in nature the budget seems somewhat high.**
Rating	**fair**

Overall

Provide a brief explanation of your summary rating.

Comments	**This proposal does not appear to be well thought out or the thoughts that went to are not properly documented.** **The proposal is to use an methodology and approach that has been applied in California for the same reason. The proposal to develop a monitorng program and adaptive management program based on the modeling effort is somewhat niave and is poorly presented. This leads to concern about utility of the work and likelihood of it's successful completion.**
Rating	**poor**

Technical Review #2

proposal title: ANADROMOUS FISH RESTORATION STUDY

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals are well−stated though I would have liked to have more detail on the historical flow patterns and temperatures if data exist.**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The project seem generally very well−justified, as recovery of the species in the mainstem San Joaguin River will clearly necessitate adequate flows and temperatures for adult migration, spawning, embryo development, fry rearing, and migration downstream. It is startling to me that such models have not been run in the decades since the dam was built but I will take the author's word that this has yet to be accomplished.**
Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to

generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The details of the models and the data on which they will be constructed have been left a bit vague. I know that these things are do−able, and that this is a reasonable approach to this kind of problem, but I wish there had been more detail on the sources of data, effort needed to massage them, etc. For example, how many years of pre−regulation flow data are available?**
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**Given adequate data and technical expertise the project seems entirely feasible. The models have been worked out and this is a matter of application of the models rather than creation of new models.**
Rating	**excellent**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**The need for a monitoring plan was noted in the proposal but monitoring was not part of the proposal per se. It is not clear whether this will be a separate proposal in the future.**
Rating	**good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The project's outputs should be sets of flows needed at different times of the year to keep the water depth and velocity and the temperature within the ranges deemed necessary. These "rule curves" or something like them will be model outputs rather than data, and as such will need to be verified by an adequate monitoring program (not specified here).**
Rating	**very good**

Additional Comments

Comments	**The rating levels (e.g., excellent, very good, etc.) are hard to apply to this project, which is such a simple and obvious one. The real issues are the monitoring, and the validity of the assumptions of adequate fish passage (and the availability of water to accomplish the goals).**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**Mr. Sutton seems to have the qualifications to do this work, though it is not entirely clear what level of training he has on these models. He is clearly a very experienced fishery biologist, who has worked in a variety of setting on different projects. No information was presented on any other staff.**
Rating	**good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**Frankly the budget seems high to me. We are told that Sutton will spend 25% of his time on the project and there is virtually no field work, equipment, etc. but the budget is over $100K. Perhaps a charge-out rate of over $100 for federal workers is standard but it seems like a lot. The amount of effort seems about right.**
Rating	**good**

Overall

Provide a brief explanation of your summary rating.

Comments	**This seems like a very much-needed (if very basic) project to determine the fesibility of having self-sustaining runs of fall chinook in this river. The methods are already established so nothing very innovative is being proposed but that is fine. The background data are described a bit too vaguely for my tastes but I would think the needed information should be available for the work to be done. In short, this is not ground-breaking in any way but nevertheless very important.**
Rating	**very good**

Technical Review #3

proposal title: ANADROMOUS FISH RESTORATION STUDY

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**There are clear hypotheses stated, but they are MUCH larger than what is actually proposed to happen in this project. The hypotheses themselves are timely and probably important, but I am unconvinced that this project will lead to their conclusion. In fact, the basic hypothesis will not be addressed by the proposal because they have no plan (stated) to implement their monitoring plan.**
Rating	**poor**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**Who knows? There is not enough background or literature review provided to make a good argument. This study would be relevant if it were paired with a larger project. The conceptual model is sketched out, but it is incomplete and not described in sufficient detail. I guess that this would fall into the category of a metanalysis using available water quanlity and flow regime data, but even the availablility of that data is not addressed sufficiently.**

Rating	**poor**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The approach has some merit. There is incredible value in modeling of this type, particularly when it serves to integrate larger data sets that have not yet been incorporated into management decisions. The results have the potential to be valuable and the project might generate a useful plan. It is simply not clear that it would happen.**
Rating	**fair**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The approach is not fully documented. There is little attention payed beyond the statement that particular models will be used. On a proposal of this scale, it would make sense to me that the applicants would at least state clearly what data is available to them and of what quality it is. On the small scale, it would seem that the project is feasible, but the stated goals are over ambitious and I question whether their results would actually be used.**
Rating	**poor**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**They state that they will design a monitoring plan, but do not describe how this will take place. They are clear that implementing the plan is not part of this project, but I am still unclear of how they will design the plan, how detailed it might be, or even who would want it (yes, I can guess, but I shouldn't have to). There is little discussion of methods (including controls, repetitions, exactly what they mean by quality control) in their discussion of the model work.**
Rating	**poor**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The report might have some value, though how it might be implemented is not discussed. There is no discussion of how it would fit with larger data management systems. There is no interpretive component.**
Rating	**fair**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The lead author has one paper using one of the models proposed while the collaborator is experienced with the temp model. The budget suggests that this project is going to be done entirely by these two individuals. There are not many other logistical issues since this is entirely a modeling project.**
Rating	**good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**It seems a bit high, but I am not sure about the time required to actually run these simulations. All costs are labor, plus a little travel.**
Rating	**fair**

Overall

Provide a brief explanation of your summary rating.

Comments	**I am not inherently negative about projects that are solely model based, but this one does not provide me enough detail to convince myself that the labor charges are warranted. I can not tell if they have the basic data necessary to run the simulations. While they make statements regarding quality assurance, they do not spell out what they intend. There are few details regarding the parameterizations of the models. They do not include any initial/preliminary analyses. They make statements about the development of monitoring plans, but do not describe this process. There is no suggested plan to facilitate the implementation of the monitoring plan. Overall, this isn't a bad idea, just a very weak proposal.**

Rating	**poor**